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Biogeosciences Discussions

Interactive comment on "Soil greenhouse gases emissions reduce the benefit of mangrove plant to mitigating atmospheric warming effect" *by* Guangcheng Chen et al.

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Dear Reviewer,

Thank you very much for your comments and suggestions. The followings are our responses to your comments. We hope the following responses and revisions made are satisfactory.

Introduction

Comment 1: You hypothesize that mangroves (the plants or the ecosystem itself?) may be important for C sequestration and mitigating global warming. Tell us what you actually tested, not the implications of your study. It sounds more like a statement

of your findings rather than a proper testable hypothesis. Response: In this study, hypothesize the mangrove plants are important for C sequestration but soil greenhouse gas emissions could partial offset the benefit of plants. According to the comment, the hypothesis were revised to (1) the mangrove soils in this region could be sources of greenhouse gas and the gas emissions would to some extend offset any benefits of mangrove plants to mitigating atmospheric warming; (2) the contributions of the two trace gases, N2O and CH4, to warming might be relevant and were considered; and (3) the mitigating effects might be spatially varied with mangrove sites.

Comment 2: Why were these sites chosen? How does this fit in with previously studied sites. Response: As some mangrove dominated shores in Jiulong River Estuary were subjected to erosion, Spartina alterniflora invasion or garbage from upstream, we chose the three mangrove sites in good conditions so as to eliminate such exogenous impacts. This has been stated in the revised manuscript (Page 5 Lines 12-14). The some background information about the Jiulong River Estuary derived from previous studies, which supports the choosing this area for study was also added in the revised manuscript; please refer to Page 4 Line 11-22.

Comment 3: Why is your research novel? Use your literature review to illustrate what this research brings to the table and why you needed to study where you did. Response: Thanks for the suggestion. Up to now, numerous studies have demonstrated that mangrove soils are significant sources of greenhouse gases, and some mangroves had intensive gas emissions. These gas emissions could contribute to the global warming, but to what extend the gas emissions would offset the benefit of plant carbon sequestration is still unclear. On base of these, the present study is novel. These have been stated in Page 3 Line 21 to Page 4 Line10. The Jiulong River Estuary is an important mangrove area in China and previous studies have reported high productivity of this mangrove. However, like many other areas, this mangrove is subjected to various human disturbances, which result in rapid mineralization processes which would produce greenhouse gases. Therefore, we though Jiulong River Estuary was a suitable

and representative area for case study. Please refer to Page 4 Lines 11-22.

Methods

Comment 1: Study area: Why were the widths of the sampling areas not all 90 meters? Why were these sites chosen, and how representative of the entire estuary are they really? You state that you have one reclaimed and one natural mangrove. What is the third site? How does the close distribution of these three sites really represent the larger area and scale up to your 'global estimates'? Response: The majority of primary mangrove forests in Jiulong River Estuary were destroyed for aquaculture activity and sea wall construction. Due to the destruction, most mangrove forests in the Jiulong River Estuary are sea-wall-fringe with narrow canopy width from their land to the sea fringes. The width of the sampling transects fit in with their canopy width, thus are not all 90 meters. In this study, we chose the three mangrove sites in good conditions so as to eliminate such exogenous impacts. The three sites locate at different areas (north-shore mangrove and island mangrove) in the Jiulong River Estuary, and cover both the rehabilitated and natural sites in this region. We therefore considered they are representative of the larger entire estuary. This is stated in the manuscript in Lines 347-349. Discussion on the global estimates was also added in the revised manuscript; please refer to Page 13 Lines 1-21.

Comment 2: Page 3, lines 26-31: How many measurements were taken at each replicate plot? Do you really have enough data points to make conclusions? What was the variation in the time of day that samples were taken while at plots during each campaign? Can you attribute any of the flux variability to the time of day measurements were taken? What about relationship to soil temperature? Response: As the sampling were all done two hours before the lowest ebb tide during the daytime; the tidal range, tidal flooding and exposure duration were comparable among sampling days and the three sites (this was stated in Page 5 Line 1 to Page 6 Line 2). Therefore the flux variability was mainly the spatial variation, rather than the time of day measurements were taken. The annual fluxes were calculated as the means of fluxes from four seasons.

This was stated in the revised manuscript. The fluxes were calculated from limited measurements (once per three month at a certain time during the daytime) and the diurnal variation was not considered. This estimation was subjected to the assumptions that the fluctuations in soil-atmosphere fluxes were insignificant during exposure period and the water-atmosphere fluxes during inundation were similar to those during exposure. The assumption was based on the following findings from previous studies (Chang and Yang, 2003; Bouillon et al., 2008; Tong et al., 2013). Although tidal effect on gas flux was observed during the exposure in previous study (Chang and Yang, 2003), the results were inconsistent. The study showed that gas fluxes in a K. obovate dominated wetland had significantly more emission after ebb tide than before flooding in August 1996; while in June 1997, the fluxes were similar between these two measurement campaigns, but the fluxes before flood were significantly higher than that after ebb tide in May and August 1998. Our preliminary study also showed that the temporal variation in gas flux was insignificant during the exposure (unpublished data). On the other hand, Bouillon et al. (2008) reported that there was no significant difference in the CO2 emission between exposed and inundated periods although the processes of gases diffused from soil to water then to air was likely affected during the inundation period. A diurnal measurement of CH4 and N2O fluxes in an estuarine marsh in Fujian Province also showed no clear difference between the inundation and exposure periods (Tong et al., 2013). Therefore, the calculations of annual emissions from fluxes during exposure time obtained from imitated measurement would not affect the findings of the present study. Please refer to Page 9 Lines 1-19 for this statement. In addition, a discussion on the effect of soil temperature on gas fluxes was added in the revised manuscript (Page 15 Lines 7-19).

Comment 3: Page 4 lines 1-10: This is a lit review of why you chose your chamber method. If it is essential to this paper, it would belong in the intro. If not vital, leave it out and just tell us what you did and cite why. Extra paragraph does not add to our understanding of your methods. Response: Thanks for the suggestion. This paragraph has been removed and the method of the gas collection was revised according to the

comments from reviewers.

Comment 4: Page 4, r12-13: The chambers had a (circular, square) area of x, a volume of y, and did not include a fan (source a, b, c). Leave out the justification or put it in the lit review. You can cite the previous research without discussing it in the methods, though I don't agree that you wouldn't need a fan. The purpose of the fan is to ensure proper mixing in the chamber over time, so while you would indeed see an increase in gas concentration over time without a fan, it would not necessarily be a correct measurement of the flux. Response: In this study, the no fan was installed in the chamber. Although the gases develop a vertical concentration gradient inside the chamber during the closure, we consider that this would not affect the flux measurement, based on the following reasons. It is generally assumed that molecular diffusion is sufficiently rapid within the chamber headspace such that homogeneous gas concentrations exist during sampling, except when large amounts of vegetation are present or the chamber volume/basal area is large (Livingston and Hutchinson, 1995). A comparison study by Moore and Roulet (1991) showed that the fluxes measured using static chambers (0.053 m2 basal area and 40cm height) has no difference from those using dynamic chamber (the insider air was circulated) with similar height. In this study, the chambers, with a volume of 1.25 l and an area of 0.025 m2, had similar volume/basal area ratio to that (1l vs.0.02 m2) used by Corredor et al. (1999), which is sufficiently small for rapid gases accumulation but large enough to minimize disturbance of the enclosed sediment surface. The height of the head space inside the chamber is around 5 cm in the present study, much lower than that used by Moore and Roulet (1991). Therefore, we consider that such gradient would not affect the flux measurements and the static chamber in this study is suitable for the flux measurement. The description of flux measurement was revised and the above clarification stated. Please refer to Page 6 Lines 5-16.

Comment 5: What were your chambers made of? Were they opaque or clear? Response: The chambers were made of plastic and clear. This is stated in the revised

manuscript.

Comment 6: Page 4, r13-18: remove "They stated. . ...is suitable for the sampling. Does not add to our understanding and is unnecessarily wordy. Response: Accept. This paragraph has been revised.

Comment 7: Page 4, r14-20: Did you use the exact same method for both CO2 and CH4? I would expect the CO2 concentrations to change much more rapidly than the CH4, and 30 minutes. My experience is with using an infrared gas analyzer in the field to measure change in CO2 concentration over a short period of time (3-5 minutes) and then use the longer (30 minutes) sampling period for slower production CH4. Can you explain why you did not do this more commonly practiced method? Response: Agree with the reviewer that CO2 change much more rapidly than CH4. The present study selected the sampling time at 10-min intervals based on the finding of previous study (Chen et al., 2010). The results showed that the linear regression relationships between the deployment time (with regular sampling at 15-20 minutes' intervals) and the greenhouse gas concentrations were highly significant (the regression coefficient values R2 were 0.963-0.999, 0.824-0.999, and 0.870-0.994 for N2O, CH4 and CO2, respectively), even in event that the gas concentrations in the chamber were very high. These indicated that gases continuously released from the sediment and accumulated in the chamber during their sampling. In this study, the CO2 flux was much lower than those reported by Chen et al. (2010) in Maipo and Futian mangroves in South China, and the dimension of sampling chamber was the same as that used by Chen et al. (2010); therefore, the build-up concentration of CO2 in the chamber would not result in significant underestimation of gas flux. The explanation of sampling interval has been included in the revised manuscript. Please refer to Page 6 Lines 18-27.

Comment 8: Page5, Sampling and analysis of soils: Why did you only sample the soils in the summer? Response: In this study we only examined the relationship in summer is because the gas fluxes have been found to be higher in summer in subtropical mangroves (Chen et al., 2012), which is conducive to this examination. As the relationship

between gas fluxes and soil parameters is not the major aims of this study, we only sampled the soils in summer. This was stated in the revised manuscript (Page 7 Lines 13-15).

Comment 9: Page5, r19 1/3 of mangrove? NPP is... Response: Accepted.

Comment 10: Page 5, r20: "A global extrapolation...." Is this your global extrapolation or a previous study? Response: this sentence was changed to "A global extrapolation by Bouillon et al. (2008) also showed a clear relationship between litter fall and wood production and further suggested that litter production amounts to ∼32% of the total mangrove NPP including root production"

Comment 11: Page6, Statistical analysis: Does your data fit a normal distribution? If not, then you should consider doing stats that aren't based on a normal distribution. Response: The normality of variables was checked using the Kolmogorov-Smirnov test in this study. The results showed that gas fluxes did not follow a normal distribution, and the fluxes were transformed to improve normality and homoscedasticity prior to analysis. This is stated in the revised manuscript (Page 10 Line 4).

Results

Comment 1: Page 6, r13: Figure 2, How did you calculate annual emissions? This should go in your methods, show an equation if necessary. Response: The annual emissions of GHGs were calculated as the means of the results from the four seasonal measurements. Detailed descriptions of the calculations are clarified in method section and in Table 3 in the revised manuscript.

Comment 2: Check your stats, F-statistics, and p-values Response: The presentation of statistical results (F statistics and p-values) were checked and corrected in the Results section.

Comment 3: Show us a graph of how the fluxes changed over the course of the year and measurements of water table depth and soil temperature would greatly improve

your insight to the heterotrophic production. We know that warm + wet = increased production, and saturation leads to anoxic conditions. Response: Agree that a soil temperature and water content are two important factors regulating the fluctuation of gas fluxes, and a graph of how the fluxes changed over the course of the year and measurements of water table depth (or soil moisture) and soil temperature would greatly helpful. However, due to the lack of such data in the present study, the graph cannot be provided in the manuscript. The effects of water moisture and temperature on soil respiration were further discussed in the revised manuscript; please refer to Page 15 Lines 7-19.

Comment 4: Table1: How did you calculate total production for liter fall? Equation and explanation, please Response: The total litter fall production was the sum of the productions of leaf, production and twitch. This is stated in the revised manuscript.

Comment 5: Table2: The stats are important, but the main point is what you are presenting as the flux values. Present both flux and CO2-C equivalent flux if you believe it is important. Response: Thanks for the suggestion. The correlation coefficient between total CO2-equivalent fluxes and soil parameters were also showed in Table 2. The results showed that the flux was related to soil NH4+-N, OC and TKN. This has been added in the revised manuscript.

Comment 6: Table3: Why are you presenting a mean if you have three sites that are not necessarily similar? Response: The mean would be helpful to understand the overall situation of the entire estuary. However, the values of the three mangrove sties were described and the spatial vitiations were discussed more in the revised manuscript.

Comment 7: Figure1: Can you put more detail into the map. A nice clean map is great, but there could be more detail that orients the reader to know what we are looking at. Maybe even a site photo to illustrate what the mangroves look like. This will help future readers know how your study is relevant to work they are conducting. Response: Thanks for the suggestion. Figure 1 has been improved and two photos showing the

canopy and the scene under canopy were also added in this figure.

Comment 8: Figure2: Consider keeping 'flux' on left y-axis and adding 'CO2-C equivalent' flux to the right y-axis as you discuss in the text. Visuals are worth 1000 words! Response: Thanks for the suggestion. CO2-equivalent fluxes for CH4 and N2O were added in Fig. 2, and Table 2 was revised with the CO2-equivalent flux of each gas removed.

Comment 9: Figure3: What season do the measurements represent? What is being shown in each diagram and table should be explicitly stated and the reader shouldn't have to refer back to the text to figure it out. Response: The sampling season (summer) was added in the caption of Fig. 3. The figure captions and table titles were improved to make them clear.

Discussion

Comment 1: I think your discussion will change, especially if you change your statistical analysis or the focus of what you are presenting in the results. It appears that the story you are telling is: "Mangrove trees sequester carbon, soils release greenhouse gases to the atmosphere. What is the balance and in the long run are these systems actually sinks or sources of C to the atmosphere?" The discussion felt overly wordy for your take home message. Response: A revision has been done for the Discussion section according to the comment, to check if the hypotheses are proved. In the Discussion, we pointed out that mangrove plants sequester carbon; soils release greenhouse gases to the atmosphere and reduce the benefit of plants, and the balance of these two processes was illustrated at the beginning. The contribution of two trace gases, the spatial and temporal variation in fluxes and their relationship with soil characteristics were discussed. At the end of the discussion, we illustrated how results of present study could be incorporated with the global carbon dynamics (including the C burial).

Comment 2: I would recommend expanding on how we can improve our estimates of mangrove sinks/sources and the implications these improved estimates on our un-

derstanding of global carbon dynamics. Response: Thanks for the suggestion. A discussion dealing the above scientific issues was added in the revised manuscript. Please refer to Page 16 Lines 3-10.

Comment 3: Compact listing of purely technical corrections at the very end (technical corrections: typing errors, etc). The majority of the grammatical and punctuation corrections can be corrected through having a native English speaker proofread the paper. There are several run on sentences and places where punctuation would improve the readability of the sentences/paragraphs. Below is a beginning list. I will be happy to read again after it has been proofread again. Response: A language edit has been done for the manuscript.

Comment 4: P1, r11: comma after greenhouse gases Response: Accept.

Comment 5: P1, r16: ". . ..among mangrove sites. Gas fluxes. . .." Gas fluxes not gases fluxes. I won't correct them throughout the rest of the paper. Response: Accept.

Comment 6: P1, r18-19: and the ecosystem was a source of methane Response: Accept.

Comment 7: P1, r26: "contribute to the global warming problem". Response: Accept.

Comment 8: P1, r29: comma after "in the past ten years" Response: Accept.

Comment 9: P2, r3: What is the actual percentage of the 'limited area' you are referring to? Give us a visual Response: According to Mcleod et al. (2011), the mangrove forest account for 0.32% of the global area of terrestrial forest. This is added in the revised manuscript. Refer to Page 19 Line 2.

Comment 10: P2, r13: anoxic conditions, which favors, microbial processes. Response: Accept with revision done.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2015-662/bg-2015-662-AC4-

supplement.pdf

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